

patient had sustained a fracture near the middle of the leg. She was a woman aged 72 years, who appeared well in health, and there was no evidence of the existence of secondary deposits.

Lastly, some tumours of epithelial origin are met with which are so slightly differentiated, so elemental, that they cannot be distinguished, except in rare instances, from round-cell sarcomata. I refer especially to the melanotic tumours which originate in connection with the pigmented epithelial cells of the rete Malpighii.

Many other facts might be quoted to show that there are tumours occupying positions intermediate between the innocent and malignant types. But these will suffice to prove that a sharp distinction, as regards their anatomical structure, cannot be made between the innocent and malignant tumours. In proportion as a tumour is more highly organised will it be more innocent, more stable, and more circumscribed; while in the measure in which its tissue is elemental or "embryonic," will it be more malignant, more diffused, and more rapidly growing. We may, perhaps, infer, from these facts, as Sir James Paget* has already suggested, that the different varieties of tumours may be produced by evolution and involution through hereditary transmission; the evolution taking place from the higher to the lower forms, as from adenoma to cancer. Considerations have also been brought forward which indicate a probability that the formation of tumours is the result of the same tendencies to variation as those giving rise to the malformations by excess, such as accessory glands or glandules, multiple exostoses, or, perhaps, even the supernumerary digits. The existence of many tumours, the evolution of which cannot be traced, should not deter us from applying this law for the explanation of the origin of morbid growths; for many types may be produced by an involution or fading away of the tumour diathesis. And the exhibition of such latent tendencies especially, may be excited only by increased blood-supply and nutritive disturbance in a part.

As a general rule, the different varieties of morbid growths appear within certain periods of life, indicated in the last lecture; and however this may be brought about, it appears probable that the nature of each variety may have been partly determined by the nutritive condition of the affected organ or tissue at the time of the formation of the tumour.

The hereditary transmission of tumours in the same form, and at approximately the same stage of life, is in accordance with the law of inheritance at corresponding periods of life; a law by which Mr. Darwin† explains the appearance of peculiarities at certain stages in the metamorphoses of insects; such as the modifications of a larva to the surrounding conditions in its structure and instincts, by which it is enabled to lead an independent existence. This law is exhibited under the normal conditions in the changes occurring at puberty, as the perfect development of the generative organs, and the growth of hair at various parts of the body; in the atrophy of the ovaries and breast at the termination of the fruitful period; and in many other phenomena, as for instance the eruption of the teeth. If we analyse the manner in which it is brought to bear we must suppose that the influence of hereditary tendency is transmitted to each and every cell in the body up to the latest period of life; and that the form and peculiarities, as well as the hereditary diseases of the individual, are the combined result of the inherent tendencies of all the ultimate elements of the body.

In the rare occurrence of cancer or sarcoma congenitally or in very early life, we see an exception to the law of inheritance at corresponding periods. This may, perhaps, be referred to the tendency in characters due to reversion to appear at an early period of life; or to one of the numerous variations in hereditary transmission. Sir James Paget tells me that he has observed cases in which the offspring of cancerous patients have manifested the disease in infancy or at a much earlier period of life than the parents. And Mr. Hutchinson, in a recent lecture, mentioned the fact, that in all the cases in which malignant tumours appear very early in life a distinct history of direct transmission can be obtained.

The following are some of the conclusions to which I have endeavoured to lead. Although some tumours appearing before puberty, and, possibly, a few even later, may be derived from rudiments formed in embryonic life, yet many others, and especially the sarcomata and cancers, originate in the tissues existing at the time of their appearance. The malignancy of these tumours may be referred to the elemental or lowly organised character of their

constituents rather than to their embryonic nature. Finally, the evolution of the different varieties of tumours from certain primary forms may be inferred; and the primary forms are allied to, and possibly originate by, the same tendencies, which, under other conditions, may produce some of the malformations by excess.

In concluding I must express my indebtedness to some authors, whose work I have not been able always adequately to acknowledge; and especially to Sir James Paget, from whose classical lectures I found it impossible to do otherwise than model some parts of Lecture II.

AN ACCOUNT OF TWO HUNDRED AND EIGHT CONSECUTIVE CASES OF ABDOMINAL SECTION PERFORMED BETWEEN NOV. 1st, 1881, AND DECEMBER 31st, 1882.*

By LAWSON TAIT, F.R.C.S.ENG.,
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ANALYSIS OF THE SERIES.

	Cases.	Deaths.	
Exploratory Incisions...	13	0	Mortality 5 per cent.
Incomplete Operations ...	8	4	
Operations for Cystoma:			
One Ovary...	36	2	112 cases; 3 deaths. Mortality 2.6 per cent.
Both Ovaries ...	28	1	
Parovarian Cysts ...	12	0	
Hydrosalpinx ...	16	0	
Pyosalpinx...	20	0	
Removal of Uterine Appendages:			
For Myoma ...	26	2	39 cases; 5 deaths. Mortality 12.8 per cent.
Chronic Ovaritis ...	12	3	
" Menstrual Epilepsy...	1	0	
Hepatotomy for Hydatids...	2	0	
Hydatids of Peritoneum ...	2	0	
Cholecystotomy for Gallstone ...	2	0	
Radical Cure of Hernia ...	1	0	
Nephrotomy for Hydatids...	1	0	
Nephrectomy ...	1	0	
Intestinal Obstruction ...	1	1	35 cases; 4 deaths. Mortality 11.4 per cent.
Solid Tumours of Ovary ...	3	1	
Hysterectomy for Myoma ...	10	2	
Cysts of unknown origin ...	1	0	
Tumours of Omentum...	1	0	
Pelvic Abscess opened and drained ...	7	0	
Chronic Peritonitis ...	4	0	
	208	16	Mortality 7.7 per cent.

When Simpson coined the word ovariectomy to describe conveniently the operations which were being done in large numbers, and with great success, by Charles Clay of Manchester, he conferred a benefit which was acceptable at the time; but he added a burden to our nomenclature which has become a real element of confusion. The word "ovariotomy," of course means cutting the ovary, not its removal, and therefore it describes an incomplete operation far better than the complete excision of the gland. By custom, its use was limited to the removal of large cystic tumours of ovarian origin, and it was a long time before solid tumours of the organ were recognised and included in the list of ovariectomies. But a far more serious mistake than this was made and is still maintained by Mr. Spencer Wells and others, by the inclusion of parovarian cysts in the list of ovariectomies. When we carefully read the descriptions of the early cases of "ovariotomy" in England, those between 1827 and 1842, we find that there is not a single real ovariectomy amongst them; they were all cases of removal of parovarian cysts. The first ovariectomy—that is, the first removal of a diseased ovary—in this country, was done by Charles Clay on September 27th, 1842.

I do not need to tell my present audience that the existence of a parovarian cyst does not necessarily involve any disease of the ovary or tube, nor that, in the great majority of cases, the ovary and tubes are not even associated intimately with the tumour. When they are so associated, they can be easily separated from the cyst, and their removal is no more necessary than is the removal of the uterus. Pathologically there is no relationship whatever between a parovarian and an ovarian cystoma; and, surgically, the only points they

* *Clinical Lectures and Essays*, p. 412.

† C. Darwin. *Animals under Domestication*, vol i, p. 75.

* Read before the Birmingham Branch.

have in common is that, for the removal of both, the abdomen must be opened and their pedicles secured. The operation for removal of a parovarian cyst has had, in my experience, no mortality at all, and has very rarely difficulty of any kind in its performance, statements which could not be made about ovarian cystoma. It is perfectly clear, therefore, that for any general statistical purpose, all cases of parovarian cyst must be eliminated from the list of "ovariotomies."

We have, then, the word "ovariotomy" limited in its application to the removal of an ovary, and the question arises, shall it be confined to the removal of diseased ovaries, or extended to include cases where the ovaries are removed for reasons which do not arise in any disease of their own tissues? No precise answer has been given upon this question; and, though I have raised it over and over again, I cannot succeed in getting it discussed. All that is done, is to apply the word "oöphorectomy" to an undefined class of cases, this word having clearly a different meaning in the mind of each person who uses it. "Oöphorectomy," curiously enough, was the first technical term used for the removal of a cystic ovary; by many foreign writers it is still used with this meaning, and, for this purpose, it is by far the more correct of the two. It is evident that, for the sake of mere common sense, two words meaning literally different things, cannot be retained to mean the same thing; and it is equally clear that the more complete and exact term of the two cannot have the less complete and more inexact application.

I am, therefore, strongly disposed to throw both words overboard, and wait patiently for a new and better nomenclature, when the present evil traditions shall have lost their hold on the professional mind. This I have already done in the case of "oöphorectomy"; and, as I have narrated my reasons at some length in every paper I have written on the subject for the last five years, I might be excused if I passed the matter over in silence, as I should do were it not that I am persistently misrepresented on this subject by those whose interest it is to misrepresent their rivals in practice. If by "oöphorectomy" is meant removal of healthy ovaries, then I have performed the operation five times, and not seventy times, as Mr. Knowsley Thornton stated recently in the *Lancet*, ignoring what I had said in the very papers he was quoting. A very large proportion, more than half of these cases, were instances of cystic disease of the ovary, and, from my records, it would be easy to place a series of cystic ovaries in order, running from 100 grains in weight up to 109 pounds. Now, who is to say, or rather who has said, at what size or weight a tumour confers upon its operation the name of ovariectomy, as against the derided "oöphorectomy?" I say here again, what I have said at least four times before, that double cystic disease of the ovaries is so frequently associated with uterine myoma, that it is perfectly impossible, in many cases, to say whether we are removing the ovaries for cystoma, or the uterine appendages for uterine myoma. Again, in cases of occluded Fallopian tubes, we constantly find the ovaries cystic; and, as the glueing of the infundibulum on to the ovary is the fundamental cause of the occlusion, we have to remove the ovary. It is an "ovariotomy" in every sense of the term, pathological and surgical; and, as the operations are admitted by everyone who has tried them to be far more serious and difficult than what Mr. Spencer Wells calls "ordinary ovariectomy," for clinical and statistical reasons, they are far more deserving to be placed with removals of ovarian cystoma than are removals of parovarian cystoma.

My conclusion grows in strength as my practice extends, that it is in surgery, as everywhere else, absolutely impossible to draw hard and fast lines of separation, and, therefore, I adhere to what seems to me to be the only just line of record, to publish every case of abdominal section I perform in its order of date, leaving my critics at perfect liberty to make what use of my cases they please, but reserving that right for myself. I must, however, protest against such criticisms as that lately made by Mr. Knowsley Thornton in the *Lancet* in the cases of Dr. Keith and myself, by the use of the figures in absolute defiance of the clear explanations given in the context.

Even for the word abdominal section, we have no clear authorised meaning. By its use, I mean an incision through all the structures of the abdominal wall, opening the peritoneum. Yet even this clear meaning must be occasionally violated, as it was in my last series, in which I published two fatal cases of removal of cysts in which the peritoneum was not opened, and in which we never knew the source whence the cysts originated. In the various columns, in each of which details are given for every case, these details are given as

fully and carefully as is within my power; but here there is doubtless great room for differences of opinion, if I may judge from the opinions I have formed from instances known to me outside my own practice. One or two illustrations will serve to indicate my meaning. Some years ago, I was present at an operation on a knee-joint performed by a very well-known surgeon. He was unfortunate or clumsy enough to divide the popliteal artery, and he had to finish his work by an amputation. In some statistics, I discovered this case figuring as an unsuccessful amputation; but was this true? Was it not an unsuccessful excision of the knee-joint? Similarly, I have an instance more to my purpose from a trustworthy eyewitness; and of it I can only say that the operation was not performed anywhere near Birmingham. For uterine hæmorrhage, it was resolved to remove the appendages. Unfortunately, the surgeon who operated has, or had, a theory that, for this proceeding, it is necessary to include the ovarian artery. He therefore planted his armed needles so close to the tumour, that, when he came to tie the ligatures, they cut through the bases of the pedicles, tore veins, and caused such hæmorrhage, that nothing remained but removal of the uterus. This case figures as an unsuccessful hysterectomy, and in a list of "oöphorectomies," where it should be, there is not a trace of it.

Similarly, concerning the word "recovery," there is apparently abundant liberty desired and taken by some recorders. I have, in the following list, one case in which there is doubt in my own mind as to whether I should rank it as a recovery or a death. It was the removal of a large cystic tumour densely adherent everywhere, and of an origin that is to this day somewhat doubtful.

At the time of the operation, the pelvis was in a condition of suppurative; the right iliac vein was completely occluded; the thigh and leg being enormously swollen, and in this condition they had been for weeks. The patient went on perfectly well till the ninth day; the wound had closed; the drainage-tube had been removed; the limb was greatly reduced in size; and she died, I would almost say suddenly, with clear indications of pulmonary embolism. I may be wrong in reckoning this as a recovery, but it is the only case where there is room for doubt; and, in support of my action, I may quote a case from Mr. Spencer Wells's last list, No. 917. At page 389 of his recent work, this case is recorded as a recovery; but it is stated that the patient died after the removal of a foreign body from the bladder. At page 336, it is explained that this foreign body was a pair of forceps, and this occurrence is regarded by Mr. Wells as inexplicable. I think an explanation would not be difficult to find; and I think that, if this case is to be regarded as a "recovery from ovariectomy," anything may be so regarded.

Here, then, we have abundant evidence to show that there is the widest possible difference of opinion as to the fundamental principles on which statistical tables are to be compiled; and, as long as this is the case, I think it perfectly needless to compare the tables of one surgeon with those of another. The real lessons to be drawn are those based on the gradual progress of each surgeon through the whole of his practice; and, unless the whole be recorded, the material is absolutely worthless. Therefore it is that I trouble you with everything that I have done. Very much of that, of course, is of comparatively little value; but, without any one case, the whole had better not be given.

The cases which are included in this series go over a period of fifteen months; and this enables me to bring my series up to the date December 31st, which closes the hospital record, a matter of convenience to myself, and one which obviates confusion. Compared with a like period in my last series, there is an increase of over 50 per cent. in the number of cases; and the total mortality is 7.7 per cent., as against 8.2 per cent. of the preceding series. The number of exploratory incisions is 6.3 per cent. as against 8 per cent., and amongst these, as usual, there is no fatal case; this experience being quite in accord with all my previous experience, that the mere opening of the abdominal cavity is a proceeding as devoid of risk as any surgical operation can possibly be. I have never lost a case; and, if all we find stated as to the omnipresence and omnipotence of germs were true, I think my experience would be somewhat different. Seven of these exploratory incisions were made merely for the purpose of ascertaining the correctness of the diagnosis of cancerous disease; and in two cancer was found where not previously suspected. In one case, the abdomen was opened for the purpose of removing chronically inflamed and adherent ovaries; a purpose it was found impossible to accomplish. In another, the operation was performed to ascertain the nature of a tumour which was found to be in the head of the pancreas. The patient was a girl aged 19, sent to me by Dr. Cunningham of Oldbury, and the

operation was performed on March 28th. Dr. Cunningham tells me the girl is now in perfect health, and the tumour has disappeared; the case being another of the mysterious cures effected by opening the peritoneum.

In my list, there are eight incomplete operations, with four deaths amongst them; this, again, being corroborative of what I have always said, that incomplete operations have a very high mortality. With two exceptions, these incomplete operations were cases of cancerous tumours. The exceptions were both, curiously enough, cysts of the mesentery, one of which died, and the other has been completely cured.

Of cystic tumours of the ovary and parovarium, I have operated in all upon seventy-six cases, with three deaths, or a mortality of 3.6 per cent., the mortality of my last series being 3.49 per cent.; and the coincidence here is so striking that I think I am quite justified in what I have repeatedly said, that in experienced hands the removal of ovarian tumours ought to have a mortality not exceeding five or six per cent. In all these cases, death seemed to be quite unavoidable. One was a case of an enormous tumour, which had been tapped sixteen times; another was a similar case in a patient aged 72; and the third was 65 years of age. I think if I had been merely regardless of my statistics, I might quite legitimately have refused to operate on the first and third of these fatal cases. All three show the dangers of delayed operations, and justify me in repeating what I have frequently said before, that if ovarian tumours are operated upon as soon as they are discovered, and *never tapped*, there would be a mortality hardly perceptible. The third case, 72 years of age, was sent to me from Bradford, and she was of such immense size that I had to tap her before she could lie down. She was very much exhausted, but rallied well in a few days. She began to fill again with great rapidity, so that the removal of the tumour had to be hastened. The adhesions were more dense than I have seen in any but one case. In this instance, I did not use a drainage-tube, and I regret it, though I do not think it would have brought about a different result. That tapping is rapidly being given up is proved by the fact that, of the other seventy-three cases, only two had been tapped. In the second fatal case, aged 65, I had to remove both ovaries, and an old myoma, recognised many years ago by Mr. Baker Brown, was left. There was no *post mortem* examination; but I suspect that, after the operation, this old tumour began to die, and its sloughing killed her. This patient also had her operation unduly delayed; so that, in all these fatal cases, I have a complete assurance that earlier operations would have almost obliterated my mortality.

Besides these seventy-six cases of what are known generally as "ovariotomies", there are two series, which include thirty-six cases without a death, which are as much ovariectomies for cystic disease as any of the previous seventy-six. They are cases of hydro- and pyo-salpinx, cysts formed by occlusion of the Fallopian tube by the infundibulum being glued on to the ovary or pelvic wall, or being closed on itself. Sometimes these cysts are of very large size, holding two or three pints, and in such a state their removal is very easy. Generally, they are much smaller, holding a few ounces; and then their removal, by reason of dense adhesions, constitute by far the most difficult class of case I ever have to deal with. Why the simplest operation of all, the removal of a parovarian cyst, should be classed as an "ovariotomy", and held up for admiration, and those most difficult cases dubbed "oophorectomies," are held up to scorn, I cannot imagine. Adding these thirty-six cases to the seventy-six, I have one hundred and twelve cases of operation for cystoma with three deaths, giving a total mortality of 2.6 per cent.

The class of cases in which relief is afforded to patients by the removal of an occluded and distended Fallopian tube, associated, perhaps, with cystic disease or chronic inflammation of the ovaries, has, as yet, received no recognition in this country outside Birmingham. There must be hundreds of women suffering in this way in London, but I have as yet heard of no operation of the kind having been performed there. A large number of my preparations are in the College of Surgeons' Museum, and I have read papers on the subject at the London societies; but the first recognition of my work has come from America. In September last, Dr. Emmett of New York paid me a visit and saw some of my cases, and on his reporting what he saw, my example at once had an influence. He took away with him some of my specimens, and on December 21st Dr. T. Gaillard Thomas reported to the New York Academy of Medicine four cases of the kind I am now discussing—tubular cysts with diseased ovaries. The symptoms in his cases are precisely those I have described, such as recurrent pelvic peritonitis, intense pelvic pain, and profuse menorrhagia, no benefit from treatment, and in-

crease of the suffering from the use of pessaries. The condition of the uterine appendages he describes just as I have described them, "ovaries covered with small cysts, and the tubes enormously distended with fluid, giving them the appearance of sausages." With equal correctness he describes the difficulties of the operation, and he gives to me an amount of praise and credit, with which it would be false modesty to say I am not deeply gratified. The complete confirmation of my work by such an authority as Dr. Gaillard Thomas is all I can desire.

My experience in these cases of chronic inflammation of the ovaries and tubes, has confirmed an impression which I have long had, that women who give way to narcotics and stimulants, have generally some strong reason for this excess, and that this is often to be found in pelvic pain. Quite a large number of the patients in these two classes have been habitual opium-eaters, and some have been drunkards, and the relief of their sufferings by the removal of the diseased organs has had the happiest results. One lady, who came to me from a very great distance, was in the most pitiable plight from the constant use of morphia. She had been taught the use of the hypodermic syringe, and her arms were covered with punctures, from the wrist far above the elbow, the skin presenting an appearance like that of chronic psoriasis. As far as I could discover, she was in the habit of injecting from five to ten grains of morphia daily, and she took an inordinate quantity of stimulants. The removal of two suppurating Fallopian tubes cured her of her pain, and she has taken no morphia and no alcohol since, now nearly twelve months.

The operation for the removal of the uterine appendages for the arrest of uterine hæmorrhage, has now received complete approval; and the statements which I first made, that after the operation not only is the hæmorrhage arrested, but the tumours shrivel and often entirely disappear, have now received complete confirmation, so that, after ten years' fighting, I feel that, upon this point, I have completely attained my object. In the present series there are twenty-six cases of this operation with two deaths, giving a mortality of 7.7 per cent. One of these deaths was due to septic poisoning, obtained indirectly from a case of scarlet fever. I need not say that I regret the incident very deeply, but fortunately my conscience is free from the responsibility of it.

Within the period embraced by the present list, I have performed only one of Battey's operations—that is, an operation performed for the purpose of influencing disease indirectly by the artificial production of the menopause in a case of menstrual epilepsy. This is the fifth of its kind I have performed, and I am as yet very reticent as to my opinion as to its value. With the exception of these cases, I may here repeat what I have said in every discussion that I have undertaken on this subject—I never operate unless I have clear physical indications of pelvic disease, in addition to any narration of symptoms I may have from either patient, or doctor, or both, unless the medical attendant will accept the entire responsibility of the proceeding. This fact has been entirely overlooked by my critics, and it has recently been the subject of a correspondence between the Editor of the *Lancet*, and Dr. Eshelby of Milford.

In a paper published in the *BRITISH MEDICAL JOURNAL* for July last, I discussed at length the symptoms and treatment of chronic inflammation of the ovary, and, therefore, I need not say here anything on the general question. During the time covered by the present series, twelve cases have been sent to me, in which the state of the patient seemed, to her medical attendants and myself, sufficiently wretched to justify the removal of the ovaries. Of these cases, three ended fatally. In two, there was no doubt accidental infection of some poisoning occurred at the time of the operation, how I do not know with certainty, but the deaths were clearly septic. In one case, I suspect the sponges were at fault, too great reliance having been placed on the virtues of carbolic acid as a disinfectant; in the other, I believe a visitor brought puerperal infection with him. Three deaths out of twelve cases is a very heavy mortality, and, if it were a permanent one, would be almost prohibitive. But I have learned from Dr. Keith, that, in the great majority of cases of death, there has been something wrong in the operation. Impressed with this belief, I am always most anxious to discover the mistake, and profit by the lesson. Tracing the cause of death in one case, as I believe, to a septic visitor, I am more than ever stringent as to who shall be present at operations; and, in the other case, having the strongest reasons to blame my sponges, I reorganised my arrangements for these dangerous articles, and now such care is exercised, that I feel sure my risks are diminished. The third death of this group was one of two cases

which gave rise to a somewhat animated discussion between Mr. Knowsley Thornton on the one hand, and Dr. Keith and myself on the other.

Mr. Thornton starts with the assumption that I either do not know, or that I will not admit, a death from septicæmia. The second alternative, amounting almost to a charge of dishonesty, is easily disposed of, for I have just indicated three such deaths. The other, a charge of careless ignorance on my part, or the assumption of omniscience on the part of Mr. Thornton, requires a little more notice; but it receives briefly an answer to the effect that neither Mr. Thornton nor I know precisely what septicæmia is. But we all recognise a peculiar form of death after operations, characterised by no constant symptoms, and by no constant pathological appearances, which used to be only too frequent, and to which, during my own lifetime at least, half a dozen different names and different theories have been attached. In my early days, it was called surgical or hospital fever; then it was called pyæmia, ichoræmia, mudanæmia, and now septicæmia; and I do not think we know a bit more about it than we did twenty years ago. Then, as now, we knew that it was due to some kind of intangible injection, bred in dirty and overcrowded hospitals, carried from one patient to another by slovenly nurses and careless surgeons, and now we are gradually banishing it by complete hygiene. When I was a student, I can remember assisting or being present at about five and twenty ovariectomies, and, I think, not one of those recovered. They were done by far more brilliant operators than I am; but these operators took none of the pains with minute details with which I burden myself morning, noon, and night. Everything was left to nurses and assistants, and no heed was given to the possibility of visitors carrying infection. Now it is all changed, and a five per cent. mortality, and the addition of numerous new operations of a kind undreamt of in my student days, are the almost daily results.

The question of Listerism has occupied a part of every paper I have read on abdominal surgery for the last six years, and I hoped that this year I should escape it; for, after trying it thoroughly, and, after having seen it practised by many others, I have, for now nearly three years, entirely discarded it, as a source of no safety in abdominal surgery, but even of considerable risk. My published experience on this subject has been followed by that of Dr. Bantock, Dr. Keith, and, lastly, by that of my colleague Dr. Savage. Mr. Spencer Wells, in his last work, has confessed that it has entirely disappointed him; and Mr. Thornton stands alone in his support of it. The arguments against it are, that Dr. Keith and I have obtained far better results without it than have been obtained with it. We have both shown that it is especially dangerous in operating upon patients with feeble kidneys; and so has Dr. Bantock: and we have quite independently expressed our belief that it will add four or five per cent. to the mortality. In the correspondence I have alluded to, Mr. Thornton says, on this point (*Lancet*, January 1st): "With regard to the action of carbolic acid on feeble kidneys, I can only say that I have not met with the dangerous results which have befallen Dr. Keith, Mr. Tait, and Dr. Bantock." But Mr. Thornton had forgotten, when he wrote this, what he had said in the same journal on June 5th, 1880. He there publishes a case, and amongst the comments occur these sentences: "It seems to be probable that the extreme congestion of the kidney was due rather to the carbolic acid than to the sympathetic irritation. It is obviously difficult to assign to each factor its share; but we know that there is often considerable congestion of the kidneys after ovariectomy, and that this has been decidedly increased in severity since the introduction of Listerism in abdominal surgery." It will be seen that in 1880 Mr. Thornton's experience was with us, but now his statement is against us. But, in the correspondence, my allusion to feeble kidneys was to Mr. Thornton's own renal organs. It is an open secret that the carbolic acid has caused him trouble, and he has operated for some time with a mask of cotton-wool over his face on this account. All I can say about Listerism in abdominal surgery is this: there are five men in this country whose published cases show they are largely engaged in the practice of abdominal section—Mr. Wells, Dr. Bantock, Dr. Savage, Mr. Thornton, and myself; and, of these, four condemn Listerism, one speaks doubtfully about it, and only one supports it. A practically similar verdict is being arrived at in Germany, and has been already pronounced in America; and, until something occurs of sufficient importance to make me reconsider my present opinions, I shall trouble myself further neither about Listerism nor Mr. Thornton.

The series concludes with a list of thirty-five cases of various operations, with four deaths. Two of these deaths occurred in cases of hysterectomy, two out of ten cases, giving a mortality of 20 per

cent.—an immense advance on Mr. Wells's mortality, as given in his recent book; for, out of thirty-nine cases, there were twenty-one deaths, a mortality of 53 per cent. This difference I attribute entirely to the use of my wire clamp, which secures circular constriction of the pedicle. In one of the fatal cases, I used the ligature; and in the other, the clamp was put too low down, and strangulated a piece of intestine. With increased experience in the use of this clamp, I believe I shall have as good results in hysterectomy as in removal of ovarian tumour. It will be seen from this that I am very much disposed to alter the opinion I expressed in my last account.

There are two cases of hepatotomy for hydatids, and two cases of cholecystotomy for gall-stone, all four patients having recovered, and being now in perfect health. It may be perhaps necessary to say that, in these and in all similar operations now recounted, there was no adhesion between the tumour and the parietal peritoneum, but the wound in the sac was stitched to the abdominal wound in the way I have previously described.

This series also includes two cases of hydatids of the peritoneum, one tumour of the omentum, one case of radical cure of umbilical hernia by opening the sac and obliterating it, three large solid tumours of the ovary, one large cystic tumour of unknown nature, one case of nephrotomy for hydatids of the kidney, and one of complete removal of the kidney already published, one fatal case of abdominal section for intestinal obstruction, four operations for chronic peritonitis, and seven for pelvic abscess. These last cases, in which I have opened the abdomen, opened and emptied abscesses, stitched the two wounds together, and drained the abscess-cavity, constitute that advance in abdominal surgery of which I am most proud. Looking back, as I am sorry to say I can do, to a large number of cases prior to February 1879, in which I dealt with pelvic abscess by the various methods then in use, so far as I can discover, more than half of these were not cured, but are either dead or continuing an invalid existence by reason of suppurating sinuses. Until I made this inquiry, I had no idea, neither I am sure has anyone else, how difficult it is to cure a pelvic abscess. I have now operated on twenty-four cases in this way, and recovery has followed in every instance. In one case, the wound never completely healed, and the patient died of pulmonary phthisis—a disease which I suspected to be in existence at the time of the operation. A second is not quite well yet, but the other twenty-two are perfectly cured.

The group of cases of which I should like to speak somewhat in detail, are those in which I have performed abdominal section on account of peritonitis, have cleared out the abdomen and drained it for a time. In the present series there are only four classed under this head, but there really ought to be nine, as that is the number upon which I operated on account of peritonitis; but finding a cause for the disease after I had got inside, the others are classed under the pyosalpinx (2), parovarian cyst (2), and pelvic abscess (1).

This illustrates again how difficult it is to make a perfectly satisfactory classification of these cases. I opened the abdomen of a lady on account of acute peritonitis, and found its cause to be acute suppurative of the Fallopian tubes. She recovered completely, and is now in perfect health. On the 7th of November last, Dr. Pike, of Malvern, telegraphed for me to go over and make an exploratory incision in a patient under his care. When I got there I found that the patient, a young lady, of 20, under the joint care of Dr. Wadhams and Dr. Pike, had symptoms of intestinal obstruction, with undoubted peritonitis. Dr. Pike had a suspicion, which he expressed before the operation, that it really was a case of acute peritonitis, from some trouble with the right Fallopian tube, symptoms of that having been in existence for two years, ever since the patient had been chilled whilst skating. Dr. Pike's diagnosis proved quite correct. I removed a large quantity of purulent fluid from the abdomen, and I found the contents of the pelvis all glued together with purulent lymph. There was no obstruction of the intestines, but the right Fallopian tube contained pus, and had burst. I removed it, drained the peritoneal cavity, and she recovered perfectly. Now the abdomen and pelvis are perfectly healthy, but within the last three weeks she has begun to suffer from some mysterious symptoms of which we cannot make any satisfactory explanation, save that they are probably spinal.

In the last account of my practice, which I published in the *Medical Times and Gazette* about a year since, I spoke of inflammation, as follows: "So satisfied have I been with results in these cases, that the next case of peritonitis, to which I am called, of whatever sort it be—even puerperal—I shall advise and perform, if allowed, abdominal section, shall cleanse out the cavity and drain it, and if the operation be not deferred till the patients are

moribund, I believe this treatment will prove eminently successful. Our views of peritonitis will, I am certain, soon undergo an immense alteration. The terms 'septicæmia' and 'septic peritonitis,' for which Mr. Spencer Wells is mainly responsible, and which have appeared in the mortality column as the explanation of the deaths after ovariectomy, are simple nonsense, and have led us astray altogether. In future we shall treat the peritoneum on the same principles as we treat other suppurating cavities, and with quite as secure results."

I have nothing to alter in these sentences. The success of drainage in saving many of the cases which formerly died, proves that if we remove from the peritoneal cavity material which is over and above its absorptive power, or if we remove, in a similar way, its own effusion, under circumstances when its absorptive power is temporarily in abeyance and threatening, with the life of the patient, to be permanently destroyed, we can put a stop to all the trouble. It seems to me that, whilst there are cases of peritonitis which are really septic, that is, when some kind of poison gets into the peritoneum and speedily affects the whole system, just as the bite of a cobra does, they are in the minority, and that the great bulk of cases are not of this character at all, but are purely local, and that if we can help the peritoneum temporarily by drainage we can secure a triumph. In the cases of chronic peritonitis, this is constantly the case. One of these from my last series will suffice to illustrate my success.

E. T., aged 18, was sent to me in April last by Dr. Justin McCarthy of St. George's, Shropshire, who kindly gave me the following details of her history. She had been for some time under the care of an irregular practitioner, and came under Dr. McCarthy's care in a state of the greatest emaciation. Seldom had he seen anyone more emaciated, unless in the last stage of phthisis—the skin drawn tightly, as it were, over the cheek bones, and all the bony prominences visible under the skin. The abdomen was enlarged, the temperature about 102°, and the pulse about 120. The chief symptoms were vomiting and diarrhoea. When she came under my care, I found her quite as Dr. McCarthy described, and the presence of a large quantity of fluid in the peritoneum was apparent. I opened the abdomen on April 17th, and removed about three pints of purulent fluid, and a quantity of white flocculent clots. I emptied and cleansed the cavity as well as I could, and fastened in a drainage-tube. The tube remained in about a week, and, during that time, a large quantity of purulent fluid and small pieces of purulent lymph were discharged. When the stitches were removed, the wound opened completely, and several large masses of this white purulent clot were extracted, one of them being as large as a normal human kidney. The wound healed in June, and about September she had completely recovered her health. I saw her on October 31st, a stout robust young woman, whom I never would have recognised as the girl who came to me only six months before.

This is the kind of operation which would have been regarded as madness about five years ago, but I think its success is enough to justify my rule concerning all these cases—"When the doctor is in doubt, and the patient in danger, make an exploratory incision, and deal with what you find as best you can."

I have not yet had such an opportunity, as for a long time I have much desired, in trying the operative treatment in puerperal peritonitis. There is not so much hope in this field for its success, as I think there can be little doubt that the majority of these cases are purely septic. But, as very few of them escape, I think it would be worth trial; and I wish some of my brethren who are unfortunate enough to see these cases would give me an opportunity of trying it. The only difficulty is, that it is like tracheotomy in croup, it must be done before it is too late, and perhaps it may be done unnecessarily. This, however, is an argument against a great deal of our practice, both surgical and medical.

I cannot close this brief and incomplete record of my most recent practice without speaking, as I have done before, in terms of the highest praise, and with unfeigned gratitude, of one to whom I owe much of my success, for his constant readiness and presence of mind in difficulty, and for his marvellous dexterity as an assistant—my friend Mr. Raffles Harnar.

[A detailed list of the cases, prepared in a tabular form, for purposes of reference and identification, accompanied the manuscript of Mr. Tait's paper, but through want of space we have been unable to publish it.—Ed. B. M. J.]

MEDICAL MAGISTRATES.—Messrs. E. T. Atkinson, and T. Carter, have been placed on the Commission of the Peace for Richmond, Yorkshire.

ON THE DIAGNOSTIC VALUE OF THE TUBERCLE-BACILLUS.

By J. DRESCHFELD, M.D., M.R.C.P.,

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NOT many months have elapsed since Koch published his researches on the tubercle-bacillus, and already we possess a number of observations showing the great diagnostic value which these pathogenic organisms have. Koch having demonstrated the presence of these bacilli in the sputum of phthisical patients, and Ehrlich having described an easy and reliable method for their detection, ready means were thus obtained to test the accuracy of Koch's observations. Leaving out of consideration prophylaxis and treatment, and considering the question simply from the diagnostic point of view, it became a matter of great importance to answer definitely certain questions.

1. Are the bacilli present in the sputa of all cases of phthisis?
2. Are they present in the earliest stages, when there are as yet few, if any, physical symptoms?
3. Are they absent in all other non-phthisical lung-affections?
4. Is there any relation between the quantity of the bacilli found or their particular stage of development and the degree and progress of the lung-affection, or, in other words, what is their prognostic value?

Answers to some of these questions have already been given by several observers: Balmer and Fränkel (*Berliner Klinische Wochenschrift*, 1882, No. 45, p. 679), Guttman (*Berliner Klinische Wochenschrift*, 1882, No. 52), D'Espine (*Revue Médicale de la Suisse Romande*, December 1882), Lichtheim (*Fortschritte der Medizin*, No. 1, 1883), and Heron (*Lancet*, February 3rd, 1883). As yet, however, the observations are few, and the results not altogether concordant. Considering the importance of the subject, and the necessity for extensive observations, I beg to be allowed to give briefly the results of my own investigations, which I commenced shortly after the publication of Ehrlich's paper, and have continued ever since.

The material used was, in all cases, the expectorated sputum, more or less fresh. (It happened repeatedly that, in the sputum of the same patient, when allowed to stand for a day or two, the quantity of bacilli found was larger than when first examined. This seems to point to a further growth of the bacilli in the sputum.) The method employed consisted, till recently, in Ehrlich's method or Gibbes's modification. Since the publication of Rindfleisch's modification of Ehrlich's method (Schill, *Deutsche Medicinische Wochenschrift*, 1883, No. 2)—which simply consists in warming the anilin-fuchsin or anilin-gentian solution, having the cover-glass with the sputum floating on it, over a flame till the fluid begins to steam—I have used this method, as it allows the whole process to be finished in five to seven minutes, and gives very satisfactory results. In many cases, the specimen so treated, after thorough washing in alcohol containing 2 to 5 per cent. of nitric acid, may be at once examined without a second staining, and shows the red bacilli standing out well in the unstained rest of the sputum; when, however, other bacilli are present, as in putrid bronchitis, or where the sputa have stood for some time, I find (contrary to Lichtheim) that the second staining with methylin-blue facilitates very much the detection of the tubercle-bacilli, if present.

Summarising briefly the results of my observations, I found the tubercle-bacilli present in all cases (forty-six) of phthisis where the physical symptoms were well marked. The patients were to a great extent hospital patients, either out- or in-patients; and their ages varied from fifteen to forty, and most of them were in the second or third stage of phthisis. In some, the disease was more acute; in others, of a chronic type. In the sputa of all, tubercle-bacilli could be detected, though the quantity varied very much, as will be shown again hereafter. In all these cases, the diagnosis of phthisis could be arrived at easily without the presence of the bacilli.

These results, so far, agree with all but one of the other observers, of whom Balmer and Fränkel found them in all their hundred and twenty cases of phthisis, D'Espine in twenty, Heron in sixty-two, and Lichtheim in all his cases but two (and in these only one examination was made, and in one case the process seemed arrested). Guttman alone states that he found the tubercle-bacilli in one-fourth of his cases only.

Of far greater importance is the detection of the tubercle-bacilli